

## ***Interactive comment on “Plankton community response to Saharan dust fertilization in subtropical waters off the Canary Islands” by G. Franchy et al.***

**Anonymous Referee #1**

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This article reports on changes in plankton community structure and primary production in the subtropical eastern North Atlantic during a 5-month period when atmospheric deposition events were frequent and intense. The topic is of interest and adequate for BG, however as explained below the manuscript in its present form does not contain sufficient evidence to establish a causal link between the dust events and the biological changes observed.

The temporal resolution of the in situ measurements is rather coarse considering that atmospheric deposition events are highly episodic and short-lived. It is possible that some of the observed changes in community structure and productivity were caused by dust deposition, but the data available do not allow to establish a clear causal link. The

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literature suggests that even strong dust events can only result in small increases of nutrient concentration (e.g. 50-200 nmol L<sup>-1</sup> of nitrate or phosphate), due to low nutrient solubility and strong dilution over the upper mixed layer. These transient increases in nutrient availability are probably followed by fast biological uptake. The question is: if the first sampling after the March dust event took place several days later, can the authors have any confidence that, for instance, the higher PP rates they measured were in fact a result of the event? Can the authors rule out the possibility that the increase in diatom biomass was due to some other factor, for instance changes in water-column physical and chemical conditions during the onset of spring?

There is no information on dissolved nutrient/metal concentration in seawater. This is a serious problem, because it prevents the authors from showing that the dust deposition event did in fact affect nutrient supply and also because the variability in community structure and productivity during the study period cannot be related to resource availability. The point is that many factors, in addition to atmospheric deposition, may have affected community structure and productivity during the study (mesoscale activity, changes in nutrient diffusion from below the thermocline, changes in irradiance, vertical mixing, etc.) but the discussion focuses only on the atmospheric forcing. The absence of nutrient and/or metal concentrations in seawater is made even more worrying when one sees in Fig. 1 that the location of the atmospheric particle collectors is quite some distance away from the location of the oceanographic observations. Seawater dissolved Al concentrations would have been very useful in assessing the magnitude and extension of the atmospheric deposition.

In the Introduction and Discussion sections, the authors emphasize the role of iron. However, their study region is not a HNLC region but a LNLC region where there is no evidence that iron is limiting primary production. The experimental work by Geider and LaRoche's groups has shown repeatedly that nitrogen and, to a smaller extent, phosphorus, but not iron, are the limiting nutrients for PP in the subtropical N Atlantic.

The primary production rates reported are impossibly high, because they imply assim-

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ilation numbers which are often higher than 20-25 mgC mgChl-1 h-1. My guess is that these rates are too high by a factor of >10. The authors are aware of the problem, but nevertheless have decided to use the data. Some error must have occurred, but there is no basis to assume that the error has been systematic and has not affected the temporal trends in addition to the absolute PP values.

#### Specific points

Why were PP measurements conducted with seawater from 20 m if atmospheric deposition affects mostly the surface layer?

Summary, 1st sentence. The authors refer to 'the high atmospheric iron, and nitrate and phosphate concentrations found in the mixed layer', which is rather puzzling. Either the concentrations are atmospheric or they were measured in seawater – not both. My understanding from the Methods section is that metal concentrations were measured in atmospheric material, not in the seawater. This should be clarified.

Methods – PP incubations lasted between 6 and 22 h, depending on the sampling date. Using different incubation times may have introduced significant error in the calculated PP estimates, because the extent to which fixed <sup>13</sup>C is respired, excreted or recycled is strongly dependent on incubation time.

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